THE

MEDICAL EXAMINER.

A

Semi-Monthly Journal of Medical Sciences.

EDITED BY N. S. DAVIS, M.D., AND F. H. DAVIS, M.D.

No. IX. CHICAGO, MAY 1, 1875. VOL. XVI.

Original Communications.

PERINEAL SECTION.

BY E. R. WILLARD, M.D., WILMINGTON, ILL.

Extract from an article read before the Will County Medical Society, April 6th, 1875.

THERE has been so much written the past ten years upon external perineal section, or urethrotomy, that it would seem a correct appreciation of the value of the operation had already become fully established by the profession.

Although such may seem to be the fact from the published accounts of cases, still, when we come to look for these cases, we find them confined to a small, select number of surgeons, and those mostly of a number who make a specialty of that department of surgery.

So far back as the seventeenth century, such men as Desault and Chopart condemned the operation on account of the great mortality attending it, and thus brought it into disrepute. In 1817 Dr. A. H. Stephens performed the operation with entire success, followed soon after by Jameson, Rodgers, and Warren, all of this country, and with the same uniform good result.

Although this was the case fifty years ago, and all well-informed surgeons of a later date must have been fully conversant with these facts, still the operation has seldom been performed since that time unless by specialists.

Prof. Van Buren says it is "one of the most difficult and uncertain proceedings of surgery."

Prof. Markae says "the dangers of the operation itself can hardly be separated from those of the disease for which it is performed," while Prof. Gross observes that "the operation is by no means free from danger, and requires the most consummate skill for its successful execution."

In looking over some notes taken in 1851, from clinical lectures of

Prof. Brainard, I find that he regarded the operation as more hazardous to the patient than that of puncturing the bladder above the pubis, or through the rectum, as instanced more particularly by a case operated upon by the former method (puncture above the pubis) by Prof. Brainard at that time. The case, a man over sixty years of age, had retention of urine thirty-six to forty-eight hours. A trocar was thrust in above the pubis, and the water drawn off. Result: the man died in a few days.

Prof. Hamilton states in his late work that "the operation will require more skill and patience than almost any other in surgery." With such authority it is no wonder the operation is confined mostly to specialists, and that most of the well-informed physicians and surgeons throughout the country are more inclined to puncture above the pubis, or through the rectum, than perform urethrotomy.

The main consideration in the performance of this operation is the danger attending it; and, so far as we are able to judge from the writings of such men as Syme, Miller, Thompson, Smith, and Gouley, the operation is not any more dangerous than that for inguinal hernia; that is, the mortality attending the operation proper only being about six to seven per cent.

Taking into consideration the above statement, together with the fact that the operation will afford permanent relief, with the certainty of ultimate cure, provided a sound is used occasionally for some time thereafter, in almost all cases treated by this plan it would seem that any other operation would not be thought of for a moment, unless the prospects for success were the same.

Before giving the following case, I would state that Prof. Gouley is entirely opposed to tying in a catheter after the operation, on account of, first, its uselessness, and second, its occasional danger. First, because the urine does not pass out through the catheter, but dribbles down by its side. Second, the irritation caused by the catheter is ten-fold more than that caused by the urine, provided the retention of the catheter would prevent the flow through the wounds.

Case ——. July 21st, 1874, I was sent for by Dr. Watson to see Mr. William Gardner, a coal miner at the B. shaft, some five miles distant. He was suffering from retention of urine, caused by stricture of the urethra, consequent upon gonorrhœa, for which he had been treated by a number of physicians at different times the past three years, none of whom succeeded in passing a catheter.

As the retention was only partial, and the bladder never distended but slightly above its natural capacity, after which a capillary stream could be voided sufficient to relieve the urgent symptoms. He had continued in this uncomfortable condition for the past three years, up to a week previous to my seeing him, at which time he became very much worse, in so much that no urine had passed the urethra for seventy-two hours. The bladder was distended to its utmost capacity, reaching above the level of the umbilicus, giving the appearance of a woman between the sixth and seventh months of pregnancy, the distention having continued so long and to such an extent that the nervous sensibility had become partially destroyed, in so much that he expressed himself as quite comfort-

able as compared with the two or three days' previous suffering. I tried to introduce catheters and sounds of various sizes, all to no purpose. I then called in Drs. Merriman, Le Caron, and Willard to assist in the operation of perineal section.

Dr. Merriman having had considerable experience in passing the capillary probe-pointed whalebone bougies, as recommended by Prof. Gouley, thought he could enter the bladder without difficulty with one of these guides. After some two hours' unsuccessful attempts by Drs. Merriman, Le Caron, and myself, to make one of these guides pass the obstruction and enter the bladder, by introducing one after another, having as many as six and seven caught in the same place at one time, we were about giving up, when I succeeded in slipping one of these guides into the bladder.

We then withdrew the other guides, and took a small-sized catheter, perforated the end, and slipped it over the remaining guide down to the stricture, with the intention of forcing it through into the bladder; but this we found to be impossible, as all the pressure the catheter was able to sustain was not sufficient to overcome the obstruction. Finding further efforts in that direction of no avail, we removed the perforated catheter from off the guide and passed a silver catheter down to the point of stricture, put the patient in the position for lithotomy, with Dr. G. E. Willard in charge of the ether, Dr. Merriman the catheter, and Drs. Le Caron and Watson to assist as occasion might require.

I made an incision one inch and a half in length in the median line of the perineum, dividing all the tissues down to the catheter, opening the urethra a little anterior to its distal end. I then introduced a small grooved directory through the perineal wound into the urethra, pressed it firmly against the stricture, placed the point of the knife in the groove, and divided the stricture freely, so that Dr. Merriman was enabled to pass the full-sized catheter into the bladder readily. The bleeding was very light, and entirely checked by sponging with cold water. The catheter was tied in and left *in situ* for forty-eight hours. Although the catheter was retained in the bladder the first two days after the operation, still the urine passed entirely by the perineal wound at its side, thus showing that the retention of the catheter was of no use. The only reasons ever alleged for retaining a catheter *in situ* for any length of time after the operation, is to prevent the urine from coming in contact with the recently cut surfaces, thereby preventing the rapid granulation and union of the wounded tissues.

In two or three days after withdrawing the catheter the urine began to pass the urethra, and in a week none came through the perineal wound, granulation being so rapid that in four weeks the wound was entirely healed. In short, the patient made an excellent and rapid recovery.

Translations.

CLINICAL THERMOMETRY.

FROM THE "COMPENDIUM DER NEUEREN MEDICINISCHEN WISSENSCHAFTEN," BY DR. B. KRAUS.

Condensed and translated by Dr. H. Gradle.

HISTORY AND SCOPE OF CLINICAL THERMOMETRY.

PROMINENT amongst the physical signs of disease, we find *animal heat*, the exact determination of which is of the greatest value in the diagnosis, the course, the prognosis, the therapeutic indications—in short, the rational treatment of disease. The importance of animal heat was known to the physicians of antiquity, and ever since their time has it been more or less appreciated. The first thermometric instrument was invented and constructed by Sanctorious, professor of medicine at Padua († 1638). After the lapse of more than a century, Boerhave made use again of the thermometer, after it had been neglected for clinical purposes for this period of time. In the year 1797 there appeared a work by James Currie, who based his therapeutic indications and success on thermometric observations. But to De Haen, the renowned professor of the clinic at Vienna, belongs the honor of being the founder of clinical thermometry. It was he who introduced the study of thermometry into pathology, who urged the employment of the thermometer at the bed-side, and adapted his treatment according to the state of the animal temperature. It was he who first considered the return of the animal warmth from the morbid to its normal standard as the true evidence of restored health. Andral has also recognized the importance of thermometry in pathology, and formulated in the year 1841 a series of definite laws on the temperature in disease.

Baerensprung, Traube, and especially Wunderlich, have within the last few years introduced thermometry into practical medicine, as they were the first who furnished careful, regular, and complete observations on the relation of temperature with diseases in their course, duration, and termination. Wunderlich especially deserves credit for his exact investigations, on the strength of which he has founded on clinical thermometry as reliable a method of diagnosis as on auscultation and percussion, and this method has attained such a reliability that, ahead of all others, it indicates the existence, degree, and importance of a disturbance of the system. By Henri Roger the thermometer has been employed in diseases of children, and a series of scientific observations on temperature in the different stages of childhood and its various diseases have been published (*Arch. Gen. de Med.*, 1844-5). Surgery, also, has resorted to the thermometer. Hunter first studied the temperature

in surgical diseases; his example was followed by Thomson, Breschet, Bequerel, and finally by Demarquay, who has made experiments on the state of the temperature during pain, hæmorrhage, the ligation of arteries, traumatic inflammations, poisoning, etc. The same surgeon, as well as Dumeril, Lacointe, and many others, has examined the influence of therapeutic agents on the animal heat.

Clinical thermometry, says Wunderlich, is an objective physical method of examination, furnishing numerical evidence of a sufficiently sensitive nature to indicate progressively the changes in the organism.

Many physicians have placed thermometry, as a means of diagnosis, above percussion and auscultation; though this is hardly applicable in all cases. That much is certain, that it yields a diagnostic basis, possessing physical exactness, and independent of the estimation or the practice and acuteness of the senses of the observer.

The determination of temperature is all the more important since, as Wunderlich correctly affirms, it is at present the only exact expression, readily observable and with sufficient minuteness, of the total alteration of the organism; hence, by following it through the course of numerous cases of the same disease, the questions can be solved whether there are pathological processes obeying definite laws in their course, what deviations from this regular course occur, and by what they are caused.

The normal temperature of the human body, almost independent of the medium in which it exists, is in itself no sign of health, though the maintenance at its normal point under different influences, as hunger, cold, bodily and mental labor, motion, etc., if they do not reach such a degree as to cause a disturbance inevitably, can be considered as evidence of a sound constitution. It is true there are pathological conditions not resulting in a measurable alteration of the animal heat—apyretic diseases—but disease exists whenever the temperature exceeds the physiological limits, and *cæteris paribus* proportionately to the extent of the deviation. By one single thermometric examination we can merely decide whether the affection is or is not febrile; occasionally a diagnosis of the morbid variety can be formed from the observation of the first few days. More frequently we exclude with absolute certainty, by means of the temperature, diseases simulated by the other symptoms. In other cases of a doubtful nature, voluntary simulation can be thus detected. But also the different individual modifications of the affection, the transition from one stage to another, the time of exacerbation or remission, the development of complications, the severity of the morbid process can be recognized earlier and more precisely by the thermometric method than by any other. As soon as a variation from the ordinary thermometric course of any disease is observed, we have received an important warning, and oftentimes the first, bidding us to search for the cause of this irregularity, thus leading not rarely to the discovery of unknown complications. At the time of convalescence, also, the temperature is the best index of recovery. Finally, the animal warmth affords us the best guide as to the efficacy or want of effect of our therapeutic procedures.

In concluding this chapter, there is yet to mention that the fear that thermometric observations could not be resorted to in private practice on account of the opposition of the patients, is not warranted after the experience in this direction with the greater part of the public. Similar apprehensions were entertained when auscultation and percussion came into use, but now every practitioner observes that patients are often not satisfied unless thoroughly examined, even more than is necessary, at each visit; and similarly thermometry, after the public has once understood its importance, will become to them a matter of course. No one can shun the slight expense of purchasing an instrument, and, as its application does not inconvenience the patient, opposition to its use can hardly be expected; even the remonstrances of children can easily be overcome with a little tact. At the same time the manner of application is so simple that any patient or nurse can manipulate it with ease, when it remains for the physician but to read off the number, which can certainly not consume much time even in large hospitals—the less as it enables the physician to dispense with other modes of examination and avoid unnecessary questioning.

PROPERTIES OF A THERMOMETER FOR MEDICAL PURPOSES.

The estimation of temperature by means of the hand is a very unreliable method, as the hand, on account of its shape, cannot adapt itself to all parts, besides it adds or abstracts warmth from the place of contact. The hand appreciates only the amount of heat it receives or loses in a given time; in other words, the difference between its temperature and that of the object examined. Thus cold can be received if the body is cooler than the hand, even though it be of a temperature above the normal point. The use of the hand gives us, therefore, but an imperfect idea of the warmth of a body, and a hint whether a more accurate determination is indicated.

For the latter purpose we require a reliable, sensitive, easily transportable and solid instrument, properties mostly possessed by the mercury thermometer. Such a thermometer, according to Wunderlich, is to be of the following description: The mercury reservoir is to have a diameter of one-third to three-fourths centimetre; if larger it loses its sensitiveness; if smaller it cannot adapt itself well to the surface of the parts. The spherical shape is preferable to the cylinder for use in the axilla; at least, the reservoir, if cylindrical, is not to possess too great a length. For the rectum and vagina a conical reservoir tapering downwards appears most suitable. A hemisphere with flat bottom has been recommended for observation of external surfaces, but gives poor results. The glass of the reservoir should neither be too thin, on account of its fragility, nor too thick, as this would reduce the sensitiveness. The tube of the instrument is to possess a uniform calibre, so small as to enable the unaided eye to divide the space between two-tenth degrees into halves and quarters. The tube is to be of such a length that the degrees ordinarily observed are at a distance of at least twelve centimetres from the bulb to permit an easy observation of the instrument while in place.* For facil-

* This description does not refer to self-registering instruments.

ity of handling, however, the entire length ought to be limited, not exceeding much the expansion of the mercury corresponding to the degrees usually attained on man. It suffices, if the scale covers but the degrees between 32.5–45° C. (90–113° F.), perhaps including also those down to 75° F. for bathing purposes, the highest being at a distance of about twelve centimetres from the bulb. A division into one-fifth degrees is sufficient for ordinary purposes.

The accuracy of the thermometer depends more on the uniform construction of the scale than on the correctness of its numbers, which latter can be regulated by comparison with a standard. If the difference between it and the standard is the same at all heights, the mere addition or subtraction of the same corrects the result perfectly. Occasional testing is to be recommended for the best thermometers for medical purposes, for the glass suffers light changes of its molecular architecture, which can affect the calibre, especially of the bulb, so that after a time the difference may amount to several tenths degrees; perhaps, also, the compression in handling the bulb, etc., may be a factor of the result. Whenever the temperature observed seems unaccounted for, a speedy testing of the thermometer is advisable.

An exceptional accuracy is attained by the use of the so-called *metastatic thermometers of Walferdin*, while for some investigations the thermo-electric apparatus appears most serviceable. The latter is constructed on the principle that an electric current is induced in a metal ring consisting of two different metals, whenever the joints are subjected to different temperatures, even though this difference be very considerable, and that this current can be indicated by the magnetic needle. Bequerel has used such an apparatus for physiological purposes. Wunderlich considers it superfluous for clinical observations. The thermograph of Marey has not yet been tested at the bedside, and will hardly ever be; it is intended to give a graphic trace of the thermometric course.

PLACE OF APPLICATION OF THE THERMOMETER.

While the thermometer can be applied to any part of the body, the most suitable place varies with the purpose of the investigation. If it be the intention to measure the temperature of a limited part, the instrument is of course to be applied at that place. If this is superficial, a thermo-electric apparatus is preferable, as the mercury thermometer is cooled off by the surrounding air unless protected. The instrument must be so applied as to be surrounded by the parts. In most cases the closed axilla appears the most suitable, as its examination does not prove inconvenient to the patient. But in emaciated or restless patients the result is not accurate; besides the temperature is always lower, and slight oscillations are not as easily observed in the axilla as in the mucous cavities, mouth, rectum, vagina, or uterus. Holding the thermometer in the closed hand is a method of no value in the determination of the bodily temperature, unless we wish to compare the extremities with each other or with the trunk.

Insertion into the buccal cavity is not a very accurate method, as the instrument is somewhat cooled off by

the inspired air; besides it interferes with the respiration of some patients, while in others it produces a tendency to vomiting. Other places of application are therefore preferable.

A more accurate plan than either of the preceding is to introduce the thermometer into the rectum, against which, however, patients frequently protest. The method is objectionable on æsthetic grounds. By the irritation defecation is occasionally induced, and an erroneous result is also obtained if the instrument enters fæcal masses. It is said, besides, that the temperature varies according to the depth, a correct result being obtained only if the bulb passes in as far as the prostate (Hunter).

Contractions of the rectum are sometimes observed altering the temperature (Billroth). It is, however, advisable to resort to the rectum as a place of application in diseases and stages in which great accuracy is necessary, and to return to the axilla when such precision becomes less important, of course noting the place of application in the tables.

The insertion into the vagina is a more accurate method than the previous, but not often applicable. It is, of course, requisite whenever the temperature of the internal genital organs is to be determined.

Winkl, Schroeder, Traube, and W. Schlesinger, of Vienna, have also made thermometric observations in the cavity of the uterus, partly to study the temperature of the female system during menstruation, pregnancy, parturition, etc., partly to determine the influence of pathological calorification on febrile temperature, and finally to diagnosticate pregnancy in the earlier months before other symptoms are developed. The possibility of such a diagnosis is warranted by the fact that the child has a temperature of its own slightly higher than that of the mother, and that the uterus is found warmer than the vagina in pregnancy, probably from the higher temperature of the fœtus. The main difficulty in these investigations is the construction of a suitable instrument, as the ordinary thermometers cannot be introduced into the womb.* Schlesinger demonstrated in the "Gesellschaft der Aertze in Wien" (session of March 6th, 1874), a thermometer for this purpose, curved at one end, the other having attached to it a scale divided into tenths of degrees. On account of its fragility, the mercury reservoir is surrounded by a metal covering, permitting its introduction into the uterus without danger. In order to avoid average temperatures, the bulb was reduced in length from three and one-half to about two centimetres, so that the cervical and corporeal cavities can be measured separately.

By means of this instrument, Schlesinger has found the difference between axilla and vagina to amount to 0.21° C. (0.38° F.), between vagina and uterus to 0.16° C. (0.29° F.), in the non-pregnant state. These results agree with those of Schroeder and Winkl, who also observed a higher temperature in the uterus than in the vagina, both in the pregnant and non-pregnant state. Further on, he found both uterus and vagina warmer than the rectum, the latter by about 0.05° C. (0.09° F.). Between the cavity of the uterus and the cervix there is also a slight difference in favor of the former.

* Not so with self-registering instruments.

Editorial Department.

ILLINOIS STATE MEDICAL SOCIETY.

THE next annual meeting of this Society, is to be held in Jacksonville, commencing on the morning of the third Tuesday in May (the 18th inst.). The place of meeting is one of the most attractive in the state, and the committee of arrangements, with their professional brethren in that city, will spare no pains to make the reception and stay of delegates and members, as pleasant and profitable as possible.

The chairmen of the several standing and special committees, from whom reports are expected, are as follows: On Practical Medicine, E. P. Cook, M.D., of Mendota; on Surgery, Edwin Powell, M.D., of Chicago; on Obstetrics, Hiram Nance, M.D., of Kewanee; on Drugs and Medicines, Wm. E. Quine, M.D., of Chicago; on Necrology, E. Ingalls, M.D., of Chicago; on Ophthalmology, E. L. Holmes, M.D., of Chicago; on Otology, S. J. Jones, M.D., of Chicago; on Diseases of Children, J. O. Hamilton, M.D., of Jerseyville; on Diseases of the Respiratory Organs, H. A. Johnson, M.D., of Chicago; on Galvano - Therapeutics, D. Prince, M.D., of Jacksonville; on Diseases of Women, J. T. Pearman, M.D., of Champaign; on Diseases of the Nervous System, J. S. Whitmire, M.D., of Metamora; on Medical Jurisprudence, R. J. Patterson, M.D., of Batavia; on Dermatology, Chas. G. Smith, M.D., of Chicago; and the Annual Address by the President, J. H. Hollister, M.D., of Chicago. This list of names is a sufficient guarantee that there will be an abundance of reports and papers to occupy the attention of the Society for three days at least. We wish the profession in every part of the state could be induced to organize societies and send delegates to the state and national organizations. It would aid much, both in elevating the character, and extending the usefulness of the whole profession.

"Each local Society shall have the privilege of sending to the Society one delegate for every five of its regular resident members, and one for every additional fraction of more than half of this number. The faculty of every regularly constituted medical college or chartered school of medicine, shall have the privilege of sending two delegates. The professional staff of every chartered or municipal hospital, and every other permanently organized medical institution of good standing, shall have the privilege of sending one delegate.

"*The Members by Invitation* shall consist of practitioners of reputable standing from any part of the United States. They shall receive their appointment by invitation of the meeting, after an introduction from any of the members present, or from any of

the absent permanent members. They shall hold their connection with the Society until the close of the session at which they were received, and may participate in the discussions without the right of voting.

"*The Permanent Members* shall consist of regular graduates from reputable schools of medicine, who have served in the capacity of delegates, or such other physicians as may be proposed by two members of this Society, reported on favorably by the committee of investigation, and receiving a two-third vote of all the members present."

A circular will be sent to each member from the committee of arrangements, giving all necessary information relative to the programme, and the arrangements made with the different railroads in regard to reduction of fare, etc.

Correspondence.

A LETTER FROM PARIS.

EDITORS MEDICAL EXAMINER: Since my last letter the number of Parisian medical journals, already great, has been increased by the appearance of the *Annales des Maladies de l' Orielle et du Larynx*. This journal is to appear every two months, and is published by MM. Ladreit de Lacharriere, physician in chief at the National Institution for the Deaf and Dumb, Isambert, physician to the hospital Lariboisiere, and Krishaber, free professor of laryngeal pathology. It will be unique in its field of investigation in France, and the editors state in their opening article that their intention is to unite in the same publication the diseases that Otoscopy, Laryngoscopy, and Rhinoscopy permit to study, inasmuch as these diseases are united by continuity of tissue. The first number contains, among other articles, an essay on the classification of the diseases of the larynx and pharynx, by Dr. Isambert; an account of the disease of Meniere and vertigo in diseases of the ear, by Dr. Ladreit de Lacharriere, and an article on rhinoscopy by Dr. Krishaber. Altogether, the journal starts under very favorable auspices and will be welcomed by the profession at large, though more particularly by the specialists.

Dr. Vibert (of Puy) has commenced a series of articles in the *Journal de Therapeutique*, on hypodermic injections of morphia, that promises to be long continued, as during a dozen years, from his own account, his therapeutics seem to have been somewhat largely confined to the use of a hypodermic syringe with a solution of morphia. From the child of seven years to the patriarch of seventy, this mode of treatment was followed, and though

space forbids his mentioning many of his cases, we find recorded a great variety of the ills that man is subject to, from erysipelas to aortic insufficiency, from metrorrhagia to asthma, all yielding under the benign influence of opium given in this manner. Yet amid all these records, that but repeat experience almost classic, we find one fact that promises to be of service in measuring the effect of morphia upon the system. This is found in his first article in the *Journal* for February 25th. From his long experience Dr. Vibert has found that unless the pupil is immobile, unchanging with the presence of a light, the patient is not fully under the influence of the drug. From the lessening mobility of the pupil the effects of the morphia can be studied with certainty, and from this action the writer states that the pupil has become for him "a veritable manometer of the action of the morphia," which indicates just when to stop the exhibition of the medicament. To illustrate, he gave a woman 8 milligrammes of morphia in order to quiet her during a violent attack of hepatic colic. Half an hour afterward the patient, who had not vomited up to that time, commenced to vomit violently, and had no diminution of her sufferings. The question was whether the susceptibility of the patient to morphia had caused this, or whether it was due to the hepatic colic. Finding the pupils still dilated, he gave another injection of 12 milligrammes. Twenty minutes afterward the pupil was two-thirds contracted, and remained so in the dark, but would still contract under the influence of a light. There was some relief, but it was only relative. Finally, the pupil still preserving about one-third of its contractility, half an hour after the second injection he gave another centigramme, which caused a permanent closure, nearly complete, of the pupil, and at the same time enough relief to permit the patient to sleep. "The modifications of the pupil had thus furnished me with precise indications for the progressive dose of the medicine, and by that had permitted me to determine it with an exactitude nearly mathematical."

It seems that the profession in France is to lend its voice against the Morse system of telegraphing, if we can judge from a report made by M. Onimus before the Societe de Biologie on March 20th. He had had occasion to observe in telegraph operators phenomena analogous to those found in Scrivener's Palsy. In one case the person had been employed as a telegraph operator for nineteen years, and ten years ago had commenced to have difficulty in manipulating his instrument. At first he found that the letters *s*, represented by three dots, *i*, represented by two dots, and *u*, represented by two dots and a dash, were no longer neatly made. At the same time he felt a sort of cramp in the hand while tracing these letters. The letter *d*, made by a dash followed by two dots, he could trace more easily than the letter *u*, which is represented by the reverse—two dots followed by a dash. The man accustomed himself to sending his dispatches by using his thumb alone. For two years he had no difficulty, but at the end of that time his thumb was affected in turn. For two months he was able to continue his work with the index and middle fingers. Finally he could only use his fist to operate with, and, the cramps returning even when he

performed his work in this way, he was forced to stop. In addition to the cramps there was a trembling of the hand and forearm when attempting to operate. Frequently he was troubled with sleeplessness and occasional signs of cerebral excitement.

Inasmuch as these accidents only arise from the use of the telegraphic signs of Morse, M. Onimus concludes that it may be necessary to abandon that system.

The indefatigable Dr. Bouchut gave a public demonstration of his cerebroscopic facts on March 16th. The little amphitheatre of the Hopital des Enfants being properly darkened for the occasion, for an hour and a half he illustrated, by means of a reflecting lamp, the appearance of the retina and blood-vessels of the eye as seen in different diseases of the brain and spinal cord. Many of the lesions had been verified by post-mortem examination, and the majority of his illustrations were taken from cases that he had had in his service since 1862, when he commenced his investigations on this subject.

The conclusions that he has arrived at as to the actual value of this means of diagnosis are published in the *Gazette des Hopitaux*, under the title of "Cerebroscopic Review for 1874." After giving in detail the history of observations made upon more than thirty cases, he concludes that the utility of the ophthalmoscope for medical diagnosis is demonstrated as follows:

When there is hyperæmia or hyperæmic swelling of the optic nerve, we can diagnose the mechanical or inflammatory hyperæmia of the brain found in meningitis, cerebral hæmorrhage, effusions in the brain, and sometimes the diagnosis can be made of spinal diseases, ataxic or other.

When he finds œdema of the papilla added to the hyperæmia, Dr. Bouchut diagnoses an œdema of the meninges or an embarrassment of the cerebral circulation that may be determined by meningitis, by certain cerebral tumors, by cerebral hæmorrhage, etc.

When there is an anæmia of the nerve in the retina and in the choroid (nervo-retinienne et choroidienne), we can diagnose the cerebral hæmorrhage of softening, and if the anæmia is complete, it is a certain sign of death. The arrest of the cerebral and cardiac circulation is shown by the emptyness of the vessels of the eye, and by the bloodless network of the choroid.

When there is an inflammation of the retina and the nerve (nervo-retinite optique), with a fatty exudation, we can diagnose a chronic meningo-encephalitis, an encephalitis that depends on cerebral tumors and the change in brain substance that accompanies these tumors.

When there are varicose veins and thrombi in the retina, there are thrombi in the meninges or in the sinuses.

Aneurisms seen in the arteries of the retina show that there are miliary aneurisms in the brain.

When there is simply an hæmorrhage in the retina, we can assert that there exists a compression of the brain from hæmorrhagic or other effusion, but if this hæmorrhage is accompanied by a fatty degeneration of the retina, it is because there is cerebral steatosis, which is the case in chronic albuminuria, leucocythemia, and diabetes mellitus.

When there is atrophy of the optic nerve, we can diagnose the presence

of tumors of the brain and of cerebral or spinal sclerosis.

Finally, tuberculous granulations are never seen in the choroid without the existence of similar changes in the lungs and other organs. In regard to the white spots that he names tuberculous granulations, it has been advanced that they are not really that in structure. Referring to this point, he argues that be their structure what it may, their presence demonstrates clinically that which he states.

An incident has occurred that does not reflect very favorably upon the Parisian priesthood. The *Journal Official* had commenced to publish a series of articles by Dr. Bouchut, in which hypnotism was discussed. While mentioning the effect of long-continued fixation of the sight upon an object near at hand, the Doctor showed that many of the remarkable experiences of olden times could be thus explained. The wondrous exhibitions that were sometimes given of fowls thrown into a state of insensibility from being forced to gaze at a chalk-mark, could easily be accounted for from fatigue of the nerves, and in the same manner other manifestations of this nature could be brought from the domain of the supernatural to that of natural phenomena. These practical views brought upon the editor of the *Journal* a host of opposition from the clergy, who denounced the publication of such heretic views. Not being able to stand against the pressure brought to bear upon him, the editor referred the matter to Dr. Bouchut. The latter finally published a letter in the *Journal*, stating that though he intended no disrespect to the traditions of the church, inasmuch as the articles seemed to create a difficulty he would discontinue them in the *Journal Official*, mentioning, however, that they would be published elsewhere; that is, in the medical journals.

Last year Pajot, professor of obstetrics and diseases of women and children, did not give his regular course of lectures, his place being supplied by a professor agrege. However, his retirement from the lecture-room being universally regretted, in response to a petition signed by all the Internes in the hospitals, and other students to the number of several hundreds, he has again commenced a course of lectures in the summer session of the school, which began on the 15th of March. The enthusiasm with which he was greeted was something immense. The grand amphitheatre of the school was crowded to suffocation. Probably there were a thousand persons inside that could hear the lecture or at least see the lecturer, while the passage ways were crowded with those that could neither see nor hear, but who testified to their feelings by following up the applause from within by hearty, if erratic, demonstrations.

The attendance does not seem to diminish with succeeding lectures, and it was only the third day that I was able to get a seat where I could hear the lecturer. The professor demonstrated the diameters and axis of the pelvis, a subject not intrinsically funny, and yet, with his curious manners and unexpected illustrations, he kept his large audience amused and attentive, not permitting the interest to flag during the hour.

Dr. Pajot is pre-eminently a teacher, and has devoted the greater part of his life to teaching rather than to the real practical life of the profession.

In fact, it is narrated of him that when a lady would ask him to attend her during an approaching confinement, he would invariably reply, "With the greatest pleasure, madame, if I do not happen to be lecturing at that time!" It is not difficult to believe that under such circumstances his private practice remained very limited.

The question of the use of chloroform in natural labor has been very hardly contested in France, and, in consequence of a paper published last year by Dr. Campbell upon the benefit of the use of chloroform under these circumstances, as illustrated by a large number of cases in his own practice, Prof. Pajot has written an article which is very severe in its opposition to such a course. It first appeared in the *Annales de Gynecologie* for January, and has since been reprinted in pamphlet form. The writer examines the question under three propositions: How do they give chloroform in natural labor? When do they give it? What are the results that they obtain?

The first question he discusses by quoting passages from the work of Dr. Campbell, to show that either they do not give enough of the chloroform to produce an anæsthesia, or that, producing the full effect, they do it at the risk of the life of the patient, as the accoucheur cannot watch the face of the mother and the descent of the child at the same time. In the latter case he accuses the supporters of this procedure of negligence. In the former case, he says they "are not serious." "It is homœopathic."

In regard to the second question, of when they give the chloroform, he states that it is only at the third period of expulsion, that being very painful, the *true* anæsthesia can be given with benefit, and then they are deprived of the possibility of directing the efforts of the mother. The advocates of this procedure do not give chloroform during the last hours of dilatation, the part of labor worst borne, but when, outside of the dangers, the indication for true anæsthesia is found.

The third question concerning the results of the anæsthesia of the advocates of chloroform in natural labor gives Professor Pajot an opportunity to show his talent for sarcasm that he does not fail to improve. He attacks them upon the ground that the pretended semi-anæsthesia is only a delusion. The indications that give warning of the approach of a period of insensibility are mentioned in this way by the advocates of this procedure: "There is a roaring in the ears that exists in nearly all the cases, and which, when it exists, appears to us to indicate that they have reached a state of semi-insensibility." Here is an opportunity that the Professor seizes with avidity, and he writes of it: "So here is the indication, precise, scientific! They ask the woman, 'Have you a noise in the ears?' 'No, sir.' 'Then you are not semi-insensible.' 'And now?' 'Yes, sir.' 'Good, you are now semi-insensible.' This recalls involuntarily the famous dialogue: 'Are you deaf and dumb?' 'Yes, sir.' 'Very good.'"

The statement that no accidents have yet resulted from the use of chloroform in this semi-anæsthesia he says should occasion the same amount of astonishment as when we fail to see fatal accidents from the use of bread pills. He contends that this means is good as a temporizer, but has no

other usefulness. To illustrate this point, he narrates an experience of his own in temporizing, that I trust I shall be pardoned for translating, as it gives a good idea of the style of Prof. Pajot:

"I delivered some fifteen years ago a primipara, granddaughter of a dowager of seventy-five years. This aged dame, person of grand airs, as noble as dry, and as dry as impertinent—rare circumstance, for old families are generally of great politeness to us poor wretches, uncouth but useful — this respectable personage assisted at the labor. The expulsion advanced but slowly, the vagina being narrow and rigid, the perineum and the vulva resisting. The good but interrupted contractions discouraged the young woman. 'My dear,' said the dowager to me (it was the first time she had ever seen me), 'one of my friends has a medal that is good for delivering all women. Shall I go for it? 'Why not, madame,' said I, with deference, only too glad for this unexpected aid. The carriage departed, and half an hour afterward, when they announced the return of the noble grandmother, I gave the patient twenty-five centigrammes of ergot prepared in advance. The medal was placed on the breast of the girl, and twenty minutes afterward the labor was ended. During an hour the dowager deafened us concerning the suddenness of this miracle, and, head under the knife, she would have proclaimed the virtues of this talisman. It has served many times since; perhaps not always with such wonderful success."

Prof. Pajot concludes his article with several propositions on the use of chloroform in natural labor, as he judges it:

True anæsthesia applied to natural labor is a serious and scientific proceeding that can be discussed. Its dangers and disadvantages appear to us greater than its advantages.

The pretended *semi-anæsthesia* is as useless as inoffensive. It has nothing scientific or serious about it. It can be placed alongside those other dilatory means that act on the imagination of the woman, and that gain time, when in natural labor there is need of nothing else. They can make women think they would have suffered more had it not been for the semi-anæsthesia. Chloroform will be for the woman in child-bed like Providence, that it is always necessary to thank when one breaks one leg—both might have been broken.

The contrast between this article of Prof. Pajot's and the one which he criticizes is striking. One is scientific, the other practical. Dr. Campbell, by experience, arrived at a result that science had failed to reach. The immunity of a woman in labor from the dangers of chloroform Dr. Campbell's experience led him to ascribe to the violent contractions that forced the blood into the brain. This is a conclusion that is not easily attacked.

Prof. Pajot's writing is brilliant, scientific, and is taken as a defence of the position of the French school against the use of anæsthetics in labor. Not a defence, to quote his own words, "by the voice of the most obscure of its professors," but one which will probably have little tendency to interfere with the actual practice of using chloroform in natural labor.

C. C. MATTESON, M.D.

PARIS, April 5th, 1875.

Society Reports.

TRANSACTIONS OF THE CHICAGO SOCIETY OF PHYSICIANS AND SURGEONS.

REGULAR MEETING, APRIL 26TH, 1875.

Reported by Ralph E. Starkweather, M.D.

THE President, Dr. John Bartlett, in the chair.

Dr. Plym S. Hayes read a carefully prepared paper upon "Experimental Observations on Salicylic Acid," of which the following is a brief summary:

After stating and reviewing the opinions of Dr. Squibb concerning this acid, and quoting from Dr. Dalton in regard to the action of the digestive fluids, Dr. Hayes criticizes the expression catalysis, and states that in the old chemistries catalysis occupied a place of great importance, and was the pack-horse on whose back was strapped many of the inexplicable phenomena connected with chemistry. The new text-books generally ignore this expression. Fermentation, which has in a measure supplanted the term catalysis, refers, in its most restricted sense, only to that action, the cause of which is almost entirely due to living organisms. Dr. Squibb calls this class of ferments "*vital*," of which that produced by yeast, and many changes which occur in putrefaction, are examples. That chemical action in which living organisms have no influence, but the metamorphosis is caused by the action of some physical agent, or of some definite chemical compound elaborated by an organism—some organic substance—the same authority calls "*chemical*," being those which occur independent of vitality, as the production of volatile oil in mustard and bitter almonds, the effects of diastase, and similar changes. Dr. Hayes details two different series of experiments, in which he used some of Dr. Squibb's unbleached salicylic acid. The first series was instituted to determine whether the acid had the power of preventing the formation of glucose when saliva had been added to starch which contained a portion of acid. The object of the second series was to ascertain whether the acid arrested or retarded in any degree the action of the gastric juice, or its active principle, pepsin, on coagulated albumen.

Next follows the account of the individual experiments which had been conducted with scholarly and scientific precision; the results attained, and the chemical agents and tests employed, being in turn re-tested and verified by other methods and processes.

The following are the conclusions drawn by Dr. Hayes: First, that salicylic acid retards digestion in a marked degree, when performed artificially. Second, that it slowly coagulates albumen.

Dr. Hayes thought that we might look for different effects in the administering of the acid, according as it

may be exhibited by the mouth or by topical application to an abrasion, provided the statements of Prof. Kolbe and Dr. Squibb are each correct. Prof. Kolbe says that given internally, no trace of the acid is discoverable in the urine or fæces. Dr. Squibb states that when the acid is applied to wounds, it appears immediately in the urine (quoting from Thiersch, *Pharm. Centrahalle*).

The paper was discussed by Drs. Bartlett and Hyde.

CANCER OF THE STOMACH.

Dr. Etheridge gave the clinical history and treatment of this case, assisted by Dr. I. N. Danforth, who had made the post-mortem examination, and exhibited to the Society, by means of the microscope, numerous pathological specimens taken from this case. The paper is reserved for publication.

The discussion which followed called forth among other facts the noticeable one that the patient, with other members of her family, had thirty years previously partaken of a broiled chicken which had been kept in a new refrigerator. The day following the meal the entire family were prostrated by acute arsenical poisoning. From that time forward the patient had been subject in summer to bilious colic, and had been obliged to refrain from certain articles of food.

Dr. Danforth thought that there had at first been simple inflammation of the pyloric opening of the stomach, and that this tissue later in life became invaded by true cancer cells. Epithelial cells, provoked from their regular growth, may become atypical, and so, after years of constant irritation, become malignant—or epigrammatically, *The lawlessness of growth is the essence of malignancy*, speaking histologically.

QUINIA AND ITS ALKALOIDS.

Mr. Albert E. Ebert, late president of the American Pharmaceutical Association, addressed the Society briefly as follows:

There are a number of alkaloids in the Peruvian bark, quinia being the best known and most largely employed. There are besides (1) quinidia, (2) cinchonidia, and (3) cinchonia. The cinchonia in the red bark is nearly as abundant as quinia.

There has been an immense amount of these "cheaper" alkaloids made while manufacturing quinia, and there having been no demand for these other alkaloids, they have been stored away and their cost has been added to that of quinia.

The cheaper alkaloids are said by reputable members of the medical profession to have the same or nearly the same therapeutical effect as quinine. The manufacturers have made efforts to introduce these bi-products (the cheaper alkaloids) to the profession, and rid their stock of these alkaloids that have accumulated for years while making quinia.

Sweet quinine was then spoken of, and the history of the exposure of the fraud, and expulsion of certain parties from the American Pharmaceutical Association.

Cincho-quinine was next referred to. If the testimonials from physicians in regard to this nostrum are true, then cinchonia sulphate has a large medicinal virtue, for none can be attributed to the quinia it is said to contain.

Mr. Wentzel, of California, a competent and conscientious chemist, has

examined this nostrum, and found that it contained cinchonia only.

In 1873, Mr. Ebert had himself examined seven different original samples, microscopically and chemically, and did not find a trace of quinia in any of them.

In the wholesale market not one of the original packages made before 1874 can be had; they have either been used or bought up.

The samples now sold do not contain the alkaloid quinia but the sulphate salt of quinia, and this only in the *six-tenths of one per cent.* (6–10 of 1 per cent.)

The success of these alkaloids goes to show that in many respects they are as useful in therapeusis as quinia sulphate, and as they cost only twenty (20) cents per ounce, are much cheaper. If extensively used the price of quinine would be reduced, and the rapid destruction from the forests of these precious trees would be much lessened.

QUININE BRUTE.

In reply to a question, Mr. Ebert said that the article generally dispensed under the term quinine brute is chinodine, or the precipitated extract of cinchona bark, such as is found in the market under the name of "Wetherill's" or "Ellis'" extract of calisaya bark.

The process is published in Parrish's Practical Pharmacy, fourth edition, p. 665.

Trousseau, in his work on Therapeutics, says that quinine brute is a combination of all the cinchona alkaloids in the impure state.

Chinoidine is the residue after the crystallizable principles have been extracted from the mother liquid, evaporated down to a solid, extractive consistency. This preparation has the same relation to the crystallizable alkaloids that molasses has to sugar.

Dr. Etheridge had found that children and others could take quinine agreeably, if given in the syrp. glycyrrhiza. In one instance, a gentleman unable to take quinine was effectually relieved by twenty-grain doses of quinine brute.

Dr. Starkweather referred to an article on intermittent fever by Bouchut, which considered that quinine brute was the most useful remedy to prescribe for children. It is less soluble and less bitter, and more easily taken by children. It should be exhibited just after the fever, in the dose of three to six grains daily. This is nearly double the dose of quinine recommended to be given to children, and adds that it should be given by enema.

The Society, after transacting considerable routine business, adjourned.

OATMEAL, BONE AND MUSCLE.—Professor Forbes, of Edinburgh, during some twenty years, measured the breadth and height, and also tested the strength of the arms and loins of the students in the University—a very numerous class, and of various nationalities, drawn to Edinburgh by the fame of his teaching. He found that in height, breadth of chest and shoulders, and strength of arms and loins, the Belgians were at the bottom of the list; a little above them the French; very much higher the English; and highest of all, the Scotch and Scotch-Irish, from Ulster, who, like the natives of Scotland, are fed in their early years with at least one meal a day of good milk and good oatmeal porridge.—*Pacific Medical and Surgical Journal.*

Gleanings from Our Exchanges.

THE CANTOR LECTURES ON ALCOHOL.

From the London Lancet, April, 1875.

THE last of six Cantor Lectures on the subject of Alcohol, its action, and its uses, was delivered by Dr. Richardson before the Society of Arts, on the 1st of February. Cantor, after whom the lectureship is named, was a member of the profession of physic, and left what may be called an Endowment to the Society of Arts, to be expended on some one or other of the useful purposes of that active and eminently serviceable public body. This session it was determined by the Council of the Society to invite public attention to the subject of alcohol and its influence on the health of the community. To be informed what effects alcohol produces on animal life was the object had in view; and, to carry this out, the council sought naturally for their lecturer a member of our body who, belonging to no particular community of reformers, and pledged to the support of no sectarian advocacy, should be able to speak with authority.

That the Society has in this measure taken a correct view is proved by the great interest that has been excited by the course of lectures now concluded. The subjects discussed have little direct reference to politics. Except in the opening sentences of the first lecture, when the lecturer touched on the one hundred and seventeen millions of money invested in the trade of alcohol in the United Kingdom, all political allusion was studiously avoided. The physician, he said, equally with the prelate and legislator, has a claim to be heard on a matter which essentially concerns the vital interests of the people; but, as the physician must restrict himself to the physiological side of the question, he cannot expect to receive the same degree of general attention as falls to the lot of more popular and favored speakers. The result has possibly for once corrected this expectation.

The subjects that have been prominently brought forward in the lectures include many in which the profession as well as the public feel an interest. The first lecture was an historical picture of the history of wine and spirit, of the origin of the processes of distillation in the laboratories of the experts of the Middle Ages, and of some additional notes on the general uses to which wine and its spirit have been applied in the service of man. The second lecture treated on the position of common or ethylic alcohol in the series of alcohols, and on the physiological action of the heavier alcohols, butylic and amylic. The third lecture described the primary physiological action of ethylic alcohol. The fourth was devoted to the question, Is alcohol food? The fifth and sixth directed attention to the physical deteriorations and mental alienations which follow the systematic and excessive use of alcoholic spirit.

The argument of the lecturer on the question advanced in the fourth lecture—Is alcohol food?—led to a negative answer, while at the same time it admitted the validity of the statement so powerfully enforced by the late Dr. Anstie, viz., that alcohol is disposed of in the living body by other processes than simple, direct elimination. This view, however, that alcohol is chemically changed within the organism, was not admitted as any proof in favor of the assumption that the substance becomes in any sense a

food. The evidence was considered conclusive that alcohol is not a builder of any tissue whatever, probably not even of fatty tissue. It therefore either becomes a food as an agent which supplies force, or it is no food whatever. If, by oxidation, it be consumed like a heat-giving food, the evidence of the fact ought to appear in the experiment of its administration. Does the evidence appear? To the solution of this question Dr. Richardson brought to bear the many facts at his command. He re-affirmed the statement originally made by him, from experimental observation, in his report to the British Association at Birmingham in 1866, that the general effect of alcohol is to reduce the animal heat. In the first stage of alcoholic excitement the external temperature of the body is slightly raised, and in the confirmed inebriate this primary elevation of temperature may be distinctly marked. But the effect is held to be due, not to a greater combustion, but to a quicker loss of temperature. It is, in truth, a process of cooling; it is from the unfolding of the larger sheet of the warm blood in vessels partly paralyzed by the alcohol, and from the quicker radiation of heat from that larger surface. During this stage the internal temperature is declining, the expired air from the lungs is indicating the first period of reduction in the amount of carbonic acid, and the reddened surface of the body is so reduced in tone that cold applied to it increases the suffusion. It is this most deceptive stage that led the older observers into the error that alcohol warms the body. In the second and third stages the temperature declines, falling below the natural; and, as the fourth stage is approached, it reaches a decline that becomes actually dangerous. This reduction is marked in some degree in proportion to the depth of the coma that follows the intoxication. With the first conscious movements of recovery, there is a faint rise of temperature, but such is the depression that these very movements exhaust and lead to a further reduction. In extreme instances so long a period as three days is required to bring back a steady natural return of the animal warmth. Through every stage, therefore, except the first, the special action of alcohol is to reduce the animal temperature. In fact, cold and alcohol act in the same manner on the vital processes. Both paralyze the vessels of the minute circulation; both lead, therefore, at certain stages of their action, to suffusion of the surface of the body, and both reduce power. In proof of this, it was shown that if two warm-blooded animals are placed asleep in the same air, brought to a low degree of temperature, the one animal charged with alcohol, the other free of the agent, they are differently affected. They both sleep under these circumstances; but the alcoholic sleeps to die; the other simply sleeps more deeply than is natural, and lives so long as the store of food it is charged with continues to support life. Within this bond it awakes in a warmer air uninjured, though the degree of cold be carried even to the degree of freezing of the extreme parts. To this evidence is added the fact that the amount of carbonic acid excreted by the animal that is under alcohol is decreased.

From all these facts the inference is that the alcohol is not burned after the manner of a food which supports animal combustion, but that it is decomposed into secondary products, by oxidation, at the expense of the oxygen which ought to be applied to the natural heating of the body.

These conclusions respecting the action of alcohol on the animal temperature were backed up by another series of observations, in which the effect of this substance was traced on the muscular power. Here it was shown that just as the animal heat is reduced so is the excitability of muscle, until at last, as narcotism is developed by the spirit, the muscular power is brought down to the extreme of prostration.

The practical deductions from these observations accord with the modern experience that is now being gained under the new light by which this

subject is surveyed. To the professional mind the facts have for some time past been gradually developing, and have afforded striking evidence of the value of properly conducted experimental research. To the public mind they are, however, new, and are so opposed to the preconceived ideas and the false experience of the past that they are startling in their novelty. So much the more important is it that the public should be correctly informed of the progress of medical science on matters which so greatly concern the general welfare. Legitimate medicine never flourishes so healthily as when an intelligent public appreciation of its labors and its knowledge is manifested and encouraged. We have touched, on the present occasion, on one point only in these lessons communicated to the Society of Arts. We shall return to them in a future number.

GENESIS ACCORDING TO SCIENCE (SO CALLED).

From the Canada Lancet.

WE cannot resist giving our readers the amusement — though the feeling excited will probably be by no means one of unmixed amusement—of reading the following smart concrete statement of some modern "scientific" schemes of creation from one of our Trans-Atlantic contemporaries:

THE NEW SCRIPTURES ACCORDING TO TYNDAL AND OTHERS.

1. Primarily the Unknowable moved upon cosmos and evolved protoplasm.

2. And protoplasm was inorganic and undifferentiated, containing all things in potential energy; and a spirit of evolution moved upon the fluid mass.

3. And the Unknowable said, Let atoms attract; and their contact begat light, heat and electricity.

4. And the Unconditioned differentiated the atoms, after its kind; and their combinations begat rock, air and water.

5. And there went out a spirit of evolution from the Unconditioned; and, working in protoplasm by accretion and absorption, produced the organic cell.

6. And cell, by nutrition, evolved primordial germ, and germ developed protogene, and protogene begat eozoon, and eozoon begat monad, and the monad begat animalcule.

7. And animalcule begat ephemera; then began creeping things to multiply on the face of the earth.

8. And earthy atom in vegetable protoplasm begat the molecule; and thence came all grass and every herb on the earth.

9. And the animalcule in the water evolved fins, tails, claws, and scales; and in the air wings and beaks; and on the land they sprouted such organs as were necessary as played upon by the environment.

10. And by accretion and absorption, came the radiata and mollusca; and mollusca begat articulata, and articulata begat vertebrata.

11. Now these are the generations of the higher vertebrata, in the cosmic period that the Unknowable evoluted the bipedal mammalia.

12. And every man of the earth, while he was yet a monkey, and the horse, while he was a hipparion, and the hipparion before he was an oredon.

13. Out of the ascidian came the amphibian and begat the pentadactyle; and the pentadactyle by inheritance and selection produced the hylobate, from which are the simiadæ in all their tribes.

14. And out of the simiadæ, the lemur prevailed above his fellows and produced the platyrhine monkey.

15. And the platyrhine begat the catarrhine, and the catarrhine monkey begat the anthropoid ape, and the ape begat the longimanous ourang, and the ourang begat the chimpanzee, and the chimpanzee evoluted the what-is-it.

16. And the what-is-it went into the land of Nod, and took him a wife of the longimanous gibbons.

17. And in process of the cosmic period, were born unto them and their children, the anthropomorphic primordial types.

18. The homunculus, the prognathus, the troglodyte, the autochthon, the terragen—these are the generations of primeval man.

19. And the primeval man was naked and not ashamed, but lived in quadrumanous innocence, and struggled mightily to harmonize with the environment.

20. And by inheritance and natural selection, did he progress from the stable and homogeneous, to the complex and heterogeneous; for the weakest died and the strongest grew and multiplied.

21. And man grew a thumb, for that he had need of it, and developed capacities for prey.

22. For, behold, the swiftest men caught the most animals, and the swiftest animals got away from the most men; wherefore, the slow animals were eaten, and the slow men starved to death.

23. And as the types were differentiated, the weaker types continually disappeared.

24. And the earth was filled with violence, for man strove with man, and tribe with tribe, whereby they killed off the weak and foolish, and secured the survival of the fittest.

"THE LEAST SACRIFICE OF PARTS" AS A LEADING PRINCIPLE OF SURGICAL PRACTICE.*

BY THOMAS BRYANT, F.R.C.S.

From London Lancet, April, 1875.

I PROPOSE, this evening, to draw your attention to what I believe ought to be a great principle of surgical practice, but to what I fear is not yet sufficiently recognized, or, if so, acted upon; I allude to the principle which, for the sake of pithiness and of practical use, I have called that of "the least sacrifice of parts." Explained more fully the principle is the one that forbids the surgeon to sacrifice more of the body than the absolute necessities of the case demand; that calls upon him to remove the disease that requires removal, but no more; that enables him in accidental surgery to make a flap for an amputation wherever he can, and in some cases to make no flap at all, but to leave the case to nature to repair; and, in pathological surgery, to cut through tissues infiltrated with inflammatory deposits rather than go above a joint, or take away more of a limb than the necessities of the case demand.

To enable me to illustrate this principle fully, I propose, therefore, to bring the subject before you in a series of propositions, each of which I hope to illustrate by cases and clinical remarks.

The propositions I hope to prove are as follows:

1. That in cases of disease or accident no more of the body is to be

* Read in part at the Medical Society of London, January 11th, 1875.

taken away than the necessities of the case demand.

2. That, to carry out this principle, the surgeon may, in *pathological amputations*, fearlessly divide tissues infiltrated with organized inflammatory products, and even cut through the walls of suppurating cavities, or through diseased joints.

3. That, in *accidental surgery*, parts irreparably injured are alone to be removed and no healthy tissues are to be sacrificed in order to perform a recognized and probably a named operation. That to these ends the surgeon ought to utilize even doubtfully viable integument, or even leave a stump to granulate, when, by so doing, some portions of the shaft of a bone can be left, a joint saved, or amputation above a joint avoided.

The first proposition—"That, in cases of disease or accident, no more of the body is to be taken away than the necessities of the case demand"—may at first sight appear a truism; and perhaps some of my hearers may be disposed to think that it is a rule generally followed. Observation, however, has taught me that such a conclusion would be far from correct, and that amputations and excisions of joints are often performed for local disease in cases from which the local disease could otherwise be removed, with far less risk to life, with an equal probability of securing a good result, and with the preservation of a limb or joint.

In the surgery of the foot or hand the truth of these remarks is well exemplified. For let me ask any hospital surgeon if it be not true that—in cases of disease of the metatarsal or tarsal bones and joints—we are too apt to regard any individual case as a good one for a Chopart's amputation, a Pirogoff's, or a Syme's, according to our own fancy or appreciation of the value of one or other of these operations; and if we are not too prone to forget that a good recovery of the foot may ensue on the removal of the diseased bone or bones without any amputation at all? And in support of this assertion I need do no more than quote the able advocate of Syme's amputation, as given in Holmes' system of Surgery (2d ed., vol. v.), where he expresses his mature opinion, after much experience, "that Syme's amputation is calculated to supersede entirely that of Chopart, besides taking the place of amputation of the leg in the majority of cases supposed to demand it." I need hardly say that I entirely dissent from these views of the learned writer. I believe that for local disease alone no form of amputation of the foot should be entertained until less severe measures have been employed and failed; that when amputation is called for, the minimum amount of foot should be taken away; that when a Chopart's amputation will suffice, a Pirogoff's should not be thought of; that when a Pirogoff's is applicable, a Syme's should not be entertained; and that an amputation of the whole foot is never to be undertaken when the disease can be removed by a less severe measure; for I cordially agree with Mr. Hancock when he asked the question, in his college lectures, "If anything can be more unphilosophical than to advocate the sacrifice of any bone or joint of the foot for no other reason than that a particular operation should be performed."

To the surgery of other parts these remarks are applicable; for do we not often see fingers amputated, and even thumbs removed, in cases of injury that, if left to nature, would very often be saved, in order that the surgeon may make a clean amputation? Do we not see joints excised that might be saved by free incisions into the joint, or the removal of necrosed bone from the joint, and amputations performed above a joint or high up a limb in order that good flaps may be made? I do not say that these things are done so commonly now as they were, but I do say that they are done far too frequently, and I believe such errors are committed because surgeons are not sufficiently impressed with the great principle of operative surgery that I have just enunciated—that of "the least sacrifice of parts."

ON METRO-PERITONITIS, FOLLOWING THE USE OF THE ORDINARY FEMALE SYRINGE: A PLEA FOR THE VAGINAL IRRIGATOR.

READ BEFORE THE OBSTETRICAL SOCIETY OF DUBLIN, FEBRUARY 13TH, 1875,
BY THOMAS MORE MADDEN, M.D., M. R. I. A.

From the Obstetrical Journal of Great Britain and Ireland, April, 1875.

THE use of the vaginal syringe is the most common, and, until recently, was almost the only local treatment resorted to in uterine disorders. As this instrument is now so generally employed in all cases of real or suspected disease of the womb, that is to say, in nine-tenths of the complaints peculiar to women, and as it is freely ordered by medical men and habitually used by patients without any special caution or apprehension of possible danger, the following case appears to me not unworthy of the consideration of this Society:

Mrs. W., a healthy young lady, aged twenty-four, was delivered on the 16th of July, 1874, of her first child. The labor, which was very tedious in the first stage, was rendered difficult by rigidity of the os uteri and malposition of the child's head, and had to be completed with the assistance of my long forceps, an instrument of a peculiar construction which I have elsewhere described.* Her convalescence presented nothing remarkable. On the 24th of August she called on me, complaining of pain in the back, a profuse yellow leucorrheal discharge, and a distressing bearing-down sensation, which prevented her from walking, or even standing with any comfort. The vagina was congested, the os patulous, and the uterus slightly prolapsed and enlarged. Absolute rest was enjoined, a tonic prescribed, and she was directed to use an astringent vaginal injection.

A few nights afterwards I was sent for, but being out of town, Dr. Egan, of Talbot street, saw her, and found her suffering from intense uterine colic, which had come on during the use of the injection when going to bed a couple of hours previously. She was then cold, almost pulseless, and apparently semi-moribund. Having promptly administered stimulants and applied sinapisms over the heart, etc., Dr. Egan, as he subsequently informed me, considered her condition so precarious, that he called in the assistance of Dr. Johnston, in consultation; and owing to the efficacious measures adopted, the most urgent symptoms were abated, the warmth was restored to the extremities, the pulse became apparent at the wrist, and the severe uterine pain was for the time considerably relieved.

On the following morning, I visited her with Dr. Egan, who kindly left the case in my care, and found her still in a state of extreme prostration, and again suffering from frequent paroxysms of violent uterine pain and almost continual retching, with great tenderness over the abdomen, and especially over the uterus, which was as large and hard as it should be immediately after delivery. Her pulse was 140, and so weak that it could hardly be counted; her respiration was sighing; countenance pale and anxious, and the skin cold and clammy.

Poultices and anodyne stupes were applied to the abdomen; hydrocyanic acid draughts were given to allay the retching, and her strength was supported by enemata of brandy and beef-tea, with small doses of liquor opii. I again saw her the same evening; the incessant sickness and uter-

* Vide *Lancet*, June 20th, 1874.

ine pain continued; she was suffering from constant thirst, but no sooner did she take any fluid, or even a piece of ice, than it was immediately rejected. She was ordered therefore to take nothing whatever by the mouth. A drop of hydrocyanic acid was added to each enema, and the expedient (which I have not seen alluded to by any writer) certainly diminished, though it did not stop the vomiting. Notwithstanding the great depression of the pulse, the local uterine inflammation was so marked, that I applied a few leeches over the seat of pain, and this gave considerable relief to the suffering, whilst the pulse actually became feebler and less compressible than before their application. Besides this, the solution of muriate of morphia was used hypodermically to relieve pain and calm the nervous system, though no sleep was induced by it.

On the 31st, the metro-peritoneal pain had in great measure subsided, but there was yet considerable tenderness over the uterus, which continued to be perceptibly tumefied. Her pulse was 140 in the minute, weak and compressible; tongue dry and furred; decubitus dorsal, and sunk down to the back of the bed, and she was still suffering much from the constant nausea and dry retching. The same treatment was pursued as on the previous day.

September 1st.—She had slept a little last night; her pulse had fallen to 120, and the uterine tenderness was much less than yesterday; the retching and inability to take any nourishment or medicine by the mouth, though less marked, still continued. I suggested a consultation. Dr. McClintock accordingly saw her with me that afternoon, and I had the satisfaction of having my view of the case confirmed by this eminent gynecologist, who, in addition to the treatment already referred to, advised oxalate of cerium, in small doses, every third hour, and Dr. Halahan's egg drink to check the retching.

September 2d.—The egg drink, in teaspoonful doses, had agreed with her, and the oxalate of cerium pills, which at first had been rejected, were now retained; it was necessary, however, to continue the enemata, etc., as before, though she gradually improved each day until the 5th, when she was able to take a little iced chicken jelly by the mouth, and from this time her recovery was rapid, so that on the 11th she was able to sit up for a couple of hours, and was soon convalescent.

In the foregoing case, the uterine colic and subsequent metro-peritonitis followed so immediately on the use of the vaginal syringe as to leave no room for any doubt of their being caused by the fluid having been injected through the patulous os into the uterus, and probably also by some of it having passed through an enlarged or dilated Fallopian tube into the peritoneal cavity.

The probability of an accident of this kind attending the use of the ordinary female syringe is practically ignored by the great majority of gynecologists; yet cases similar to mine, although somewhat rare, have been recorded by other practitioners; but they would appear to have made less impression on the minds of those engaged in this branch of medicine than the importance of the subject demands.

Dr. Tilt says: "Only once have I been led to believe that the patient injected some portion of the fluid into the cervical canal. A lady was suffering from chronic uterine inflammation; the womb was low and slightly retroflected; the os uteri patulous; and after injecting a solution of acetate of lead, as the patient thought, in the usual way, she was suddenly seized with severe uterine pains, rigors and intense cold. She got better when in bed, by means of abdominal poultices and hot drinks, and no bad consequences followed this attack." *

Dr. J. H. Bennet says: "When disease really exists in the uterine

* Dr. Tilt's "Handbook of Uterine Therapeutics." Third Edition. P. 54. London, 1868.

cavity, the injections would, no doubt, do much good, and were they safe, would be preferable to the solid nitrate of silver applied with the porte-caustic; but there is reason to believe that uterine injections are not safe, and I do not now resort to them. Several deaths occurred in Paris during my residence there, from metro-peritonitis, brought on by their use. One took place in the female ward of M. Jobert, in the Hospital St. Louis, and under my care, as I was then his house-surgeon. The patient, a fine healthy young woman of twenty-four, was afflicted with a large fibrous tumor of the uterus, which had much developed that organ, and had, no doubt, opened the os internum. M. Jobert was at that time trying the effects of the so-called uterine injections, and injected one astringent injection into the cervical canal of this young female, there being a slight muco-purulent discharge from the os. Shortly after she was seized with rigors, fever, and severe abdominal pain, and in a few days died of peritonitis. I performed the post-mortem and found nothing but the lesions of peritonitis, and the ovarian tumor embedded in a womb developed to the size which it presents in the fourth month of pregnancy. The fluid of the injection must have penetrated freely into the uterus, through the open os, and thence have passed along the Fallopian tube into the cavity of the peritoneum, thus causing fatal peritonitis." †

M. Bernutz relates the history of a case of peritonitis "which resulted immediately from the administration of a cold vaginal douche." ‡

These three quotations are the only references that I have met with to the dangers which may follow the use of the ordinary vaginal syringe, and the paucity of similar observations is certainly a fair argument that the accident in question is by no means frequent. Still the mere possibility of such serious, or even fatal results, being thus produced, should, I think, render gynecologists more cautious than is generally the case in their recommendation of this almost universally-employed and much-abused instrument. For my own part, I have been long convinced of the inconvenience of the vaginal syringe in ordinary use, as well as of the possible ill effects of its misapplication. Some years ago I pointed out in this place how uncertain and undue might be the force with which injections can be thus thrown into the vagina or into the uterus, and I endeavored to show that, even when not positively injurious, this instrument must necessarily be imperfect in its action, and inconvenient in its use. To produce any permanent beneficial effect by injections, in a case of congestion or inflammation of the cervix uteri for instance, the injected fluid must be kept in contact with the inflamed or congested part for a certain fixed length of time, and at a certain unvarying degree of heat, without any sudden alternations in either its temperature or the force with which it is impelled. None of these intentions can be carried out when the common siphon syringe is made use of, as the fatigue of working that instrument, and the position of the patient, which is so irksome during its employment, effectually prevent its being used more than a few minutes continuously. Moreover, the injected fluid is sent by it into the vagina in irregular intermitting jets, the force of which is directed against the inflamed part with a degree of violence that may be injurious, or of weakness that may be utterly ineffective, and which is regulated by the strength or patience of the operator, rather than by the necessities of the case. In order to obviate these difficulties, various forms of utero-vaginal syringes, douches, and irrigators have been devised. It may be in the recollection of some present, that a couple of years ago, I exhibited a very simple,

† "On Inflammation of the Uterus and its Appendages." By J. H. Bennet, M.D. Fourth Edition. P. 146. London, 1861.

‡ "Clinical Memoirs on the Diseases of Women." By MM. Bernutz and Goupil. Translated by Dr. Meadows. Vol. II. P. 78. London, 1868.

and, I believe, very effective instrument, for the same purpose before the Society. The irrrigator in question* (a description and drawing of which may be seen in the second volume of our proceedings, p. 183), is one I have now used for some years, and which, without asserting that it is in any way superior to other instruments of the same kind, I have found more generally useful than the ordinary syringe. With this irrigator, the accident which forms the subject of the present paper, could not have occurred, as it is merely a siphon, so constructed that it can be set in action, or stopped in an instant at the will of the patient.

It has, moreover, the great advantage of being very portable; may be readily used whenever a vessel of water can be obtained; is capable of sending a gentle continuous current of water, plain or medicated, and at any temperature, into the vagina, or even into the uterine cavity itself, if ever that measure—rarely required in gynecological, as distinguished from obstetric practice—should be considered expedient, and this, too, in any position, sitting or recumbent, and for any length of time that may be advisable, without causing the slightest fatigue to the patient. The advantages of the irrigator over the common syringe, I have proved by experience during the last few years, and the comfort and benefit which those patients who have employed it have derived from its use, would lead me to recommend any gynecologist who has not hitherto done so, to give a trial to the irrigator as a substitute for the vaginal syringe in most cases. This instrument is not only effective and easily used, but is also easily constructed. Dr. Graily Hewitt, Dr. Kidd, and several others, have recommended and devised irrigators which many would, perhaps, prefer to the one I have brought before this Society. But it is in the power of anyone to convert an ordinary vaginal syringe into an excellent irrigator by the mere addition of two pieces of india rubber tubing and a small stopcock to it. These may be attached to any syringe, which may be thus made, perhaps fully as effectual as the most complicated instrument that could be devised for the purpose.

In another point of view, cases such as those just referred to, appear to me of some interest, namely: as to their bearing on the recent discussion as to the safety of strong astringent injections into the uterus immediately after delivery, in order to arrest hæmorrhage. Against this practice, it has been urged that there is danger of forcing the injected fluid through the open uterine sinuses into the circulation, or through the Fallopian tubes into the peritoneal cavity, thus causing, in the former case, death from embolism, and in the latter giving rise to fatal peritonitis. As on a former occasion, when this subject was under the consideration of the Obstetrical Society, I expressed a strong opinion against the probability of such an accident being thus produced, I feel bound to say that the case I have now related has, to some extent, modified my views on this point. For if metro-peritonitis may be occasioned, as it was in this instance, by a fluid injected into the uterus five weeks after delivery, it is obviously much more probable that a similar effect might be thus occasioned immediately after parturition, when the uterine vessels and passages are so enlarged and pervious. But I should also add that the possibility of such an accident does not in the least alter my opinion of the utility of the powerful styptic brought into midwifery practice by Dr. Barnes, nor would it deter me from again resorting to the injection of the solution of perchloride of iron in any case of post-partum hæmorrhage which could not be otherwise controlled.

The President observed that Dr. Madden's brief paper opened up a very important subject for discussion. He did not think the occurrence of uterine colic following the injection of fluids by the syringe, was a very rare occurrence, inasmuch as he had seen

* Made by Whyte, Upper Sackville Street, Dublin.

three cases of it in his own practice. In one case only a few drops of glycerine were injected into the cavity of the uterus, as recommended by Dr. Marion Sims, and it produced most intense colic, but no peritonitis or endo-metritis followed. Some two years ago he was called, late at night, to see a patient whom he had directed to use a weak solution of borax injected into the vagina with an ordinary syringe. He found her in a state of collapse, suffering from pain referred to the uterus, and sickness of stomach. Her symptoms were speedily relieved, and no inflammation followed; while a less severe attack occurred in a patient who used tepid water only. He thought these cases in which the injection of a fluid into the uterus was followed by colic, were far from being of very rare occurrence, and he advised that the central hole in the nozzle of the syringe be stopped, as a means of preventing this accident. He did not think, however, that the data given by Dr. Madden carried out his theory that the fluid passed into the Fallopian tubes, and thence into the peritoneum. The phenomena in Dr. Madden's case might be explained by the occurrence of a severe attack of endo-metritis in the first instance, followed by peritonitis. The exact same train of symptoms which Dr. Madden had described—the prostration, collapse, and vomiting—occurred in a patient where he (the President) had swabbed out the uterus with perchloride of iron. The patient was suffering from profuse hæmorrhage occurring some weeks after abortion; the os was patulous, and he had no difficulty in passing a pledget of cotton, saturated with the styptic, into the uterus; this was followed by a train of symptoms exactly similar to those Dr. Madden had described, but it was impossible that the fluid was passed through the Fallopian tubes. Certainly Dr. Madden was quite right in saying that vaginal injections were not perfectly free from danger. He (the President) greatly preferred a douche, similar to that spoken of by Dr. Madden, to the use of a vaginal syringe, and he had recently (acting on the suggestion of Dr. Emmet, of New York,) carried out that plan extensively. Dr. Emmet advocated strongly this vaginal irrigation with water, varying from 95° to 105° temperature. He based his treatment on the analogy that existed between the effects produced by it and an ordinary linseed poultice, or other hot application, applied to the skin. He says: If you leave a linseed poultice on for some time, the skin becomes corrugated and white, showing that, though the first effect of the warmth was to attract the blood to the surface, this was followed by contraction and diminution in size of the calibre of the blood-vessels. He (the President) did not think this analogy held good, for, in the case of the application of a poultice four or five hours elapsed before bloodlessness of the part occurred, while the vaginal douche could hardly be prolonged over fifteen or twenty minutes. Therefore, although considerable benefit followed in many cases from the hot vaginal douche, he did not think the theory was quite correct. To use the vaginal douche efficiently, it was necessary that the hips be raised higher than the shoulders, so that the vagina may be distended with the fluid; that the stream of fluid should be continuous, and that it should be kept up to an equal temperature. He (the President) had tested this method extensively. In a case of metritis, where the temperature of the water was raised to 110°, the patient derived much benefit. In another, a case of vaginitis, the patient had been subjected previously to treatment of various kinds without benefit. Two gallons of water, temperature 105°, were used twice daily. She derived the greatest relief, and was, in fact, cured by it. The great difficulty in carrying out this treatment, where a large quantity of water necessarily is used, was that the bed-pan on which the patient lay had to be emptied constantly. To obviate that difficulty he had introduced the plan of attaching a tube to a bed-pan of peculiar shape, by which

means the water was carried away. The pan must be placed on a firm surface, and, if this precaution is taken, no trouble whatever is experienced in using it. The water for the douche should be contained in a case such as that used by surgeons for irrigation, and be raised to the height of a few feet above the level of the couch or table on which the patient rests.

HETEROPLASTY.—M. B. Anger has communicated to the Academy of Sciences of Paris a paper wherein he mentions that he has succeeded in transplanting large pieces of skin taken after recent amputations, upon large ulcers and extensive losses of substance after burns. This he calls hetoroplasty. The author uses the whole thickness of the skin, and takes care that the temperature of the part to be transplanted be not low. In fact he has laid patients side by side so as to convey the skin of one without the least delay upon the ulcer of the other. M. Anger is very careful to say that the general health of those patients who furnish skin should be thoroughly investigated before the operation.—*London Lancet.*

Obituary.

THE LATE DR. ADDISON NILES.

DIED, at his residence in Quincy, Ill., April 5th, 1875, Dr. ADDISON NILES, aged 63.

Dr. Niles was born at Plattsburgh, Steuben county, N. Y., April 26th, 1812. He commenced the study of medicine while young, though exactly at what date, cannot now be remembered; but he studied and practiced with his father, Dr. Noah Niles, for some years before graduating. He first attended lectures in 1833–4, at the medical college of Geneva, N.Y., where he graduated in 1835, after attending two courses. He subsequently attended another course at Geneva, in 1845–6; and during the following winter (1846–7), we find him at Jefferson Medical College, Philadelphia. He resided in Plattsburgh, Avoca, and Bath, of Steuben county, N.Y., until 1855, when he removed to Lockport, in Niagara county. Finally, in November, 1861, he removed to Quincy, Ill., at the solicitation of friends residing in this city, where he resided, engaged in a busy practice until his death, which occurred on the evening of April 5th, 1875.

Dr. Niles may be said to have lived solely in and for his profession, to which he was thoroughly and devotedly attached; few — of his medical friends, at least—ever heard him discourse upon any other subject. He was an indefatigable laborer in the cause of science, and possessed probably the largest and best selected medical library in this section. He had been, in former years, a contributor to some of the medical journals, and there is little doubt that he left a large amount of manuscript in the form of notes, essays, etc., which being founded on

the experience of over forty years of practice, and containing in all probability, many peculiar and original ideas, cannot but be of interest; and as the doctor's son, Mr. John W. Niles is engaged in the study of medicine, it is to be hoped that these valuable papers may not be allowed to rest in oblivion.

In his intercourse with his professional brethren, Dr. Niles was somewhat reserved; though courteous to all, he cultivated but few friends or intimate acquaintances, but he was highly respected and admired by those who knew him. To his patients, and especially to those of the poorer class, among whom he had many, he was ever kind and tender; the numbers that filled St. Peter's church at the funeral service, would alone suffice to tell in what esteem he was held by those who attended the last sad rites.

Of his last illness, but little is known, for with his habitual reserve he consulted none, and refused all aid; when his family, alarmed by his condition, requested counsel, those who were called, arrived but to find him on his dying bed. An autopsy held the following morning, revealed extensive organic lesions, that had probably existed for many years; a full account of this will be given by Dr. M. Rooney, in a report to the Medico-Pathological Society.

The loss occasioned by the death of Dr. Niles, will long be felt, and more especially by the Fellows of the Medico-Pathological Society, of which he was one of the founders, and in which he was an ardent and enthusiastic worker. May his memory serve to stimulate his surviving Fellows, to renewed and never-failing exertion in the path which he so lately trod, and in the labors from which he is now at rest.

At a special meeting of the Medico-Pathological Society of Quincy, Illinois, held this day, the following preamble and resolutions were unanimously adopted:

"WHEREAS, We have learned with profound sorrow, of the death of DR. ADDISON NILES, one of the original Fellows, and a past President of this Society; therefore

"*Resolved*, That in his death, we mourn, in common with the entire medical profession of the city and county, the loss of one whose untiring devotion to science might well be held up as an example to his Fellows; whose unbending integrity commanded our fullest respect, and whose unfailing courtesy in all his professional relations, had won our highest esteem.

"*Resolved*, That we tender to the family of the deceased, that sympathy which comes from hearts suffering with them a common bereavement, and cherishing with them the honored name of him who has gone before."

It was ordered, that a copy of the above resolutions be sent to the family of the deceased, and that they be published in the Quincy *Whig* and *Herald*, and in the Chicago MEDICAL EXAMINER; also, that they be inscribed on the memorial pages of the Society's records, with such facts as may serve as a synopsis of the medical history of the deceased.

L. H. COHEN, M.D.,
Secretary.

QUINCY, Ill., April 6th, 1875.